

these relics are pre-glacial. The most one can say about them is simply this, that the folk who used them lived in England before the climax of glacial cold. When human relics are got in beds of older date than those at Cromer, we shall then have a demonstration of the pre-glacial age of man in Britain. At the same time the presumption is (as many geologists will admit) that some portion of our ancient river-drifts and cave-deposits with flint implements do really belong to pre-glacial times. In short, after carefully reading the proceedings at the recent Conference, I find nothing to shake me in my present belief that none of the palæolithic deposits belongs to post-glacial times, but that all must be relegated to inter-glacial, and probably pre-glacial ages, and consequently that the palæolithic is separated from the neolithic age by the intervention of the last cold period of the glacial epoch. My opinion, therefore, is still as strong as ever that "until we clearly understand what was the succession of changes during the ice age, it is premature to speculate upon the geological age of those deposits which yield the earliest traces of man in Britain." In concluding, may I be allowed to suggest to the anonymous writer whose communication on the subject of the Antiquity of Man appears in the number of this journal for June 7, that before he again essays to criticise my views he might do well to become better acquainted with them. JAMES GEIKIE  
Perth, June 15

BEFORE your readers accept the statements of Messrs. Evans and Hughes respecting my discovery of flint tools beneath the great chalky boulder-clay, as announced in NATURE last year, may I ask them to remember that as yet I have not published the evidence upon which I founded my statement? The delay has arisen from official and other causes; and although my paper is now written, it is, I have just learned, too late for reading during the present session at the Geological Society. Neither of the two gentlemen named is aware of the extent of my evidence, for I have not, as yet, told any one about it, except the two geologists mentioned below. As I shall show, there are now known to me about forty localities in which the brick-earths in question occur, and in most of them their relation to the boulder-clay is very clear; and even in the two or three spots in which that rock is not seen in the actual section, it overlies the implement beds near by on the same outcrop. The brick-earths have naturally suffered much denudation by the boulder-clay, and I have a splendid series of sections showing every phase from almost undisturbed material beneath the boulder-clay to small fragments (boulders, in fact) in that deposit. I wish, also, to state, that instead of four implements from two localities, as originally announced, I know at present nearly 150 from six different spots. The evidence is so clear and overwhelming when seen *en masse*, that it must be convincing to all who carefully weigh it. The boulder-clay which overlies the brick-earths in question is part and parcel of the great mass of the chalky boulder-clay, a formation which I have spent eight years in examining in the field almost daily, of which I have mapped about 2,000 square miles, and upon which I feel quite competent to form an opinion.

Prof. Prestwich is perfectly correct in ascribing the well-known palæolithic implements found in the gravel to a time subsequent to the formation of the chalky boulder-clay; but that *only proves those tools to be newer than the last glaciation of this particular area*. Now inasmuch as Mr. Searles V. Wood, jun., long ago proved, and as everybody who examines the ground must admit, that the "purple" and "hessle" boulder-clays are newer than the one we are dealing with, and as Dr. J. Geikie has shown that gravels bearing the same character and possessing the same peculiar fauna as the well-known palæolithic gravels (overlying, moreover, the chalky boulder-clay), pass under these newer beds, it is, to say the least, a misnomer to call these gravels *post-glacial*. They are post-glacial to this East Anglian area, but not to northern England; and the distribution of surface-beds containing palæolithic implements throughout Europe shows that they are confined exclusively to that area which was free from the erosive action of the newer and less intense ice-sheets of the latter part of the "Great Ice Age." Much of the misapprehension in this matter has arisen from the unfortunate name of "upper" given to the chalky boulder-clay. It is "upper" in East Anglia, but it is "lower" in Yorkshire.

My discovery does *not* prove man to have been pre-glacial; it merely shows that he was "pre-chalky-boulder-clay," and I last week obtained evidence to show that the brick-earths in question belong to the "middle glacial" of Mr. Searles V. Wood, jun.; that is to say, they are newer than the Cromer till, but older than the chalky boulder-clay.

Mr. Belt is mistaken respecting the quartzite implements near Brandon. They are found in gravel which is unquestionably above the boulder-clay, as can be seen in hundreds of sections, and the only conceivable source of that material is the boulder-clay. We have, in fact, two horizons of palæolithic implements, one above and one below the boulder-clay, and I am in hopes that the former will be found capable of subdivision, for many facts crop up in the course of my daily work which seem to point in that direction.

Prof. Ramsay and Dr. J. Geikie, who are eminently capable of judging of glacial phenomena, have gone over the area with me, and are perfectly convinced of the accuracy of my determinations. SYDNEY B. J. SKERTCHLY

Brandon

#### Nicephore Niepce

THERE is an error in one of your "Notes" of last week which you may be glad to have corrected. It is not to Niepce de St. Victor that the citizens of Chalons-sur-Saône (a town, by the way, not to be mistaken for Chalons in the Champagne country) are about to erect a statue, but to his uncle, Joseph Nicephore Niepce, who might well be designated as the first photographer, since he it was who succeeded first of all in fixing an image in the camera. In a "Life of Nicephore Niepce,"<sup>1</sup> recently published by Victor Fouqué, appear letters which leave little doubt that in May, 1816 Niepce had accomplished the feat of fixing shadows in the camera, for in a communication of that date to his brother he incloses four photographs, of which he says: "The pigeon-house is reversed on the pictures, the barn, or rather the roof of the barn, being to the left, instead of the right. The white mass which you perceive to the right of the pigeon-house, and which appears somewhat confused, is the reflection upon the paper of the pear-tree, and the black spot near the summit is an opening between the branches of the trees. The shadow on the right indicates the roof of the bake-house." This, then, is a description of the first camera-picture ever taken, and it was by reason of Niepce's inability to prevent his impressions from fading after a lapse of time that he turned his attention to the bitumen of Judea process, with which he produced photographs as early as 1824, one or two specimens being still among the science treasures of the British Museum.

The name of Nicephore Niepce is little known in England. And yet this should not be. As is well known he came to this country in 1827, and resided at Kew in the hope to receive aid and encouragement, and shortly afterwards, on his return to France, entered into partnership with Daguerre to work out together a more practical process. When Daguerre made known his discovery in 1839, his partner had been dead two years, and no mention was made of Niepce at the time Arago made his famous speech announcing the discovery of Daguerreotype. Specimens of the wonderful process were not long in reaching this country and the first picture was placed in Faraday's hands with the remark that he had never seen anything like it before. But Faraday said he had. A Frenchman, he remembered, had brought him a picture of Kew Church a dozen years ago, with the quaint remark, that "the sun had done it." Faraday was so certain of this that inquiries were at once instituted into the matter, and in the end, a communication was addressed by the Secretary of the Royal Society, Mr. Bauer, to the *Académie* at Paris, a communication which helped materially to substantiate the claim of the Niepce family, and to obtain for the son Isidore, a pension in acknowledgment of the father's services. The deed of partnership between Niepce and Daguerre is still extant, but how much of the latter's published results were due to his dead partner the world will never know. H. BADEN PRITCHARD

June 16

#### Japanese Mirrors

THERE is still something to be solved about the Japanese mirrors, which show the figures that appear in relief on the back in the disk of light reflected from the face. Not only does it seem impossible (without some indication which I have not yet met with) to tell which mirrors in a series will perform the feat, but it is equally difficult to say why one bunch of leaves will, and another in the same casting, will not appear in the spectrum.

In reference to Mr. Hignley's quotation of Mr. Prinsep's conjecture (p. 132), "that the thinnest parts, from being the hardest,

<sup>1</sup> "La Verité sur l'Invention de la Photographie."

should give the strongest reflection, owing to a difference in density produced by stamping," and to the brassworker's solution, not only are the mirrors not stamped, but cast; but it is the thicker parts, those which stand out on the back in highest relief, which reflect the most light. I have one on the back of which there are two large characters as it were laid upon a background of trees. These letters have been carefully ground flat and polished like the front. Their figures not only appear more distinctly than those of other less highly raised adornments, but actually, in the sun, throw off a brilliant white light, which contrasts very markedly with the comparatively subdued spectrum from the rest of the plate.

Is it possible that there may be some difference in molecular arrangement during the consolidation of the melted metal in the thicker (relieved) and other parts of the plate? And yet, the thick rim of the mirror does not reflect a rim of light.

One of my specimens has suffered a little oxidation, and I observe that this appears on the face to follow certain of the larger masses of relief on the back. This mirror does not "show the pattern through," but there is a curious bright rim reflected from the edge of each scar of injured surface.

Manchester

R. D. DARBISHIRE

#### Colour-Sense in Birds

As the fact of the preference of sparrows for *yellow* crocuses still excites interest and requires explanation, perhaps you will allow me to call attention to the following remarks of Gilbert White in his "Observations on Nature":—

"Birds are much influenced in their choice of food by colour, for though white currants are a much sweeter fruit than red, yet they seldom touch the former till they have devoured every bunch of the latter."

The obvious criticism that the craving for sweets which distinguishes the human biped is not equally predominant among his feathered friends, and consequently, that their selection of the less sweet but more highly coloured fruit may be due to some taste other than the æsthetic, does not detract from the importance of White's generalisation that birds are much influenced in their choice of food by colour—a generalisation which, there is no reason to doubt, was based upon his own keen and repeated observation.

PAUL HENRY STOKOE

Beddington Park

#### OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich mean times of visible geocentric minima of Algol, for July, August, and September, according to the elements adopted by Prof. Schönfeld:—

h. m.			h. m.			h. m.		
July 11	11 51	...	Aug. 20	15 11	...	Sept. 12	13 39	
" 31	13 31	...	" 23	11 59	...	" 15	10 28	
Aug. 3	10 20	...	Sept. 9	16 51	...	" 18	7 16	

Minima of S Cancri occur on September 8 at 15h. 1m., and September 27 at 14h. 14m.

A minimum of Mira Ceti will fall on July 23, according to Argelander's formula of sines, the same perturbations being applied as in the case of the maximum of the year, which is computed to occur November 97.

Mr. John Tebbutt, writing from Windsor, N.S. Wales, on April 13, states that in consequence of remarks on the probable variability of  $\mu$  Doradus, in *NATURE*, vol. xv. pp. 14 and 281, he examined the star on February 26, and March 14, and found it of the 8th magnitude. There is a star, estimated 9th magnitude, about thirty seconds of time west, and twelve seconds north of it. With such an instrument as was employed by Lacaille at the Cape of Good Hope in 1751,  $\mu$  Doradus, with its present brightness, would hardly have been visible. Lacaille calls it a fifth magnitude.

MINOR PLANETS AND COMETS OF SHORT PERIOD.—Dr. von Asten, in the course of his recent researches on the motion of Encke's comet, found that, although in the interval 1819-68 the comet had experienced in each period of revolution an almost exactly equal amount of acceleration, and that this might be attributed to the existence of a resisting medium, yet in order to connect the last two appearances in 1871 and 1875 with the previous ones, it

is necessary to have recourse to the hypothesis of an extraordinary perturbation which, in the period 1868-71, counteracted the influence of a resisting medium. For certain reasons Dr. von Asten is led to conjecture that about the middle of the year 1869, when the comet was in the region occupied by the numerous group of small planets (the radius-vector being about 3.2), it made so close an approach to one of these bodies, as yet undiscovered, that a sensible effect on the comet's mean motion was the result.

In connection with this hypothesis it may be interesting to note that the late Prof. Hubbard, whose masterly investigations on the motion of Biela's comet appeared in Gould's *Astronomical Journal*, came to the conclusion that the separation of the comet into two distinct bodies, by whatever cause effected, took place in all probability in a heliocentric position corresponding to about longitude  $318^{\circ}6$ , latitude  $+12^{\circ}0$ , with radius-vector 4.36, which position the comet occupied in November, 1844 (*Ast. Journ.*, No. 140). It is stated in some works that the comet in 1846 separated under the very eyes of astronomers; nevertheless it is upon record that the companion was first recognised on December 29 by Herrick and Bradley at New Haven, but was not again seen until Maury refound it on January 13; and its not having been remarked when the comet was first glimpsed in the Northumberland and other powerful telescopes may well have been owing to distance and faintness.

A radius vector of 4.36 would, until quite recently, have been considered as placing the comet rather outside the probable superior limit of distance of the minor-planet group, but the discovery of Hilda by M. Palisa in November, 1875, considerably extended the limit, this body in aphelion being distant from the sun 4.6. Although the separation of Biela's comet, if it really took place at the epoch assigned by Prof. Hubbard, could not have been owing to an encounter with this particular planet, yet the position indicated for the occurrence is clearly a possible one for a meeting with an unknown member of the group. In saying this much we are of course aware that the separation may have been owing to a very different cause, indeed it might be supposed that such a *rencontre* would have left a more sensible effect upon the mean motion of the comet.

METEORIC FIRE-BALLS IN AMERICA.—Prof. Daniel Kirkwood in a communication to the American Philosophical Society, on March 16, gives some particulars of meteoric fire-balls which appeared in unusual number in the United States in the latter part of 1876 and beginning of the present year. The circumstances attending the appearance of eight conspicuous meteors are included: the dates were 1876, July 8 (two fire-balls), December 16 and 21, January 3, 20, and 23, and February 8. The train of the larger meteor of July 8 was visible at least forty minutes, the mass having been apparently dissolved or dissipated in the latter part of its track; the motion about the sun was retrograde, but sufficient materials were not forthcoming for determining the orbital velocity or the nature of the orbit. The fire-ball of December 16 had been visible but a few seconds near San Francisco when it apparently plunged into the Pacific at no great distance from the shore, the fall being followed by a loud detonation. The meteor of December 21 was remarkable for the length of its track, between 1,000 and 1,100 miles, one of the longest upon record, and moreover, the track would appear to have been somewhat curved. When crossing Indiana the principal fire-ball was followed by a train of smaller meteors, many of which exceeded Venus and Jupiter in apparent magnitude; the breadth of the cluster, as seen from Bloomington, was  $3^{\circ}$ , and the length at least  $20^{\circ}$ , from which Prof. Kirkwood concludes that the true diameter was five miles; and the length about forty miles; several explosions occurred during the passage of the meteorite over Indiana and